Stylised fact or situated messiness? The diverse effects of increasing debt on national economic growth

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Abstract

This paper reanalyses data used by Reinhart and Rogoff (2010c - RR), and later Herndon et al. (2013) to consider the relationship between growth and debt in developed countries. The consistency over countries and the causal direction of RR's so called 'stylised fact' is considered. Using multilevel models, we find that when the effect of debt on growth is allowed to vary, and linear time trends are fully controlled for, the average effect of debt on growth disappears, whilst country-specific debt relations vary significantly. Additionally, countries with high debt levels debt appear more volatile in their growth rates. Regarding causality, we develop a new method extending distributed lag models to multilevel situations. These models suggest the causal direction is predominantly growth-to-debt, and is consistent (with some exceptions) across countries. We argue that RR's findings are too simplistic, with limited policy relevance, whilst demonstrating how multilevel models can explicate realistically complex scenarios.

Keywords: Public Debt, Economic Growth, Multilevel Modelling, Reverse Causality

JEL codes: E60, C23, H63

Introduction

A recent paper by two American economists on 'Growth in a time of debt' – an in-house working paper (Reinhart and Rogoff, 2010a) and an abbreviated version published in the annual conference *Papers and Proceedings* volume of the *American Economic Review* (Reinhart and Rogoff, 2010c) – has attracted much comment since its publication. Its subject was the relationship between a country's debt (relative to GDP) and the associated annual rate of real GDP growth. Its conclusions appeared to offer strong support for the austerity budgets being pursued by a number of countries following the credit crash and banking crisis of 2008 (on which see Lysandrou, 2013, Rogoff and Reinhart, 2013).

The first set of comments reflected the important substantive and policy implications of their findings, especially those based on a dataset applying to 20 OECD countries for the period 1947-2009. Their core conclusion (Reinhart and Rogoff, 2010c, 573) was that:

> … whereas the link between growth and debt seems relatively weak at "normal" debt levels, median growth rates for countries with public debt over roughly 90 percent of GDP are about one percent lower than otherwise; average (mean) growth rates are several percent lower. [In this, a reduction of one percent should be interpreted as a reduction of one percentage point.]

This was considered 'timely' given that 'Public debt has been soaring in the wake of the recent global financial maelstrom'. It was interpreted – by journalists and other commentators (no doubt following the authors' lead) and then by policy analysts – as suggesting the existence of a 'debt burden cliff': once the ratio of debt to GDP exceeded 90 per cent, then a country's rate of growth fell off substantially. At that time, austerity budgets were being promoted in the UK and other European countries, and politicians seized on Reinhart and Rogoff's conclusion to sustain arguments that their country's debt:GDP ratio should be kept below that key threshold in order to ensure substantial economic growth to lift the country out of recession. Indeed the draft budget drawn up for the United States by Representative Paul Ryan (later Mitt Romney's running-mate in the 2012 US Presidential election) was similarly based on that 'stylized fact' (Reinhart and Rogoff, 2010b, 1).

The second set of comments was initiated in spring 2013 with the publication of a working paper from the Political Economy Research Institute at the University of Massachusetts-Amherst (Herndon, et al., 2013). As a graduate student, Herndon undertook an assignment to repeat the analyses of a key paper – in his case Reinhart and Rogoff's – and found that he could not replicate their findings with the dataset used in the publications referred to above. In particular – after exhaustive analyses and re-analyses – it appeared that there was no evidence of a 'debt burden cliff': the rate of growth declined as the debt:GDP ratio increased (as shown in Figure 1, which replicates Figure 1 in the Herndon et al. paper), but there was no significant change in the relationship at or close to the supposed 90 per cent threshold (or indeed any other). The linear regression line shown in Figure 1 is very shallow, and the addition of quadratic and cubic terms provided no evidence of a significant downturn for the fourth group.

[Fig. 1 about here]

This conclusion reflected three criticised elements of Reinhart and Rogoff's work:

- The exclusion of data for certain years in three countries, which included several of the key years there when the debt:GDP ratio exceeded 90;
- The exclusion of all data for five countries at the top of the alphabet due to a spreadsheet error; and
- Some weakly-justified averaging-cum-weighting decisions that substantially affected the outcome.

Correcting for the errors and exclusions, and removing the weighting and averaging procedures, Herndon et al. (2013, 1) concluded that:

> … when properly calculated, the average real GDP growth for countries carrying a public-debt-to-GDP ratio of over 90 percent is actually 2.2 percent, not -0.1 percent as published by Reinhart and Rogoff. That is, contrary to RR, average GDP growth at public debt/GDP ratios over 90 percent is not dramatically different than when debt/GDP ratios are lower.

The policy prescription based on Reinhart and Rogoff's analyses was thereby undermined: there was no empirical rationale for maintaining that the debt:GDP ratio should not be allowed to exceed 90 per cent.

Herndon et al.'s careful (failed) attempt to replicate Reinhart and Rogoff's analyses unsurprisingly had a very substantial impact on economic commentators, who drew potential implications for future policy directions in those slow-growing countries where austerity budgets had been promoted politically as the necessary way forward. Alternative policies based on greater investment in growth stimuli, even if they took the debt:GDP ratio over 90, had been dismissed by those politically-committed to austerity budgets: the veracity of their arguments was now in doubt. (Indeed, at least one commentator (Linden, 2013, 2) has suggested that 'The key argument that high debt causes slower growth has crumbled' and 'Countries around the world have experimented with austerity, and those experiments have failed spectacularly'.)

The paper does, moreover, fit into a wider literature considering the macroeconomic relationship between growth and debt, the conclusions of which are somewhat mixed (for a more comprehensive review of this debate, see Panizza and Presbitero, 2013). A "conventional view" separates out the long and short term effects of debt, with debt being beneficial in the short term but in the long run leading to declines in national savings because of future repayments (Elmendorf and Mankiw, 1999). This could be exaggerated if debt leads to uncertainty among investors fearing default (Cochrane, 2011a, 2011b), or if debt-overhang (Krugman, 1988) means that countries have difficulty borrowing further. However, it is also argued by some that, because of the negative effects of prolonged recession on future economic performance, borrowing money to stimulate growth can often be worthwhile (Cerra and Saxene, 2008, DeLong and Summers, 2012). The idea of finding a balance between the two, and thus there being an 'optimal' level of debt, provides a theoretical basis for non-linearities, where the growth-debt relationship is hump-shaped (Checherita-Westphal et al., 2012). However, empirically, both the presence and shape of any relationship is dependent on the specification of the model and the statistical method being used – certainly the pro-austerity message taken by many from Reinhart and Rogoff (2010c) has been overstated.

The Herndon et al. paper carried two important messages for social scientists seeking to influence policy through their empirical analyses. The first – very clearly – was to be sure that your data are correct and any manipulations fully justified. This has been widely trawled in the media and through social networks, and is not the subject of further discussion here. The second message is highlighted by another of their conclusions, which has attracted much less attention because their main goal was to show that Reinhart and Rogoff's findings could not be replicated, using their methodology but with corrected data. Herndon et al. also found that '… the relationship between public debt and GDP growth varies significantly by time period and country' (p.1): in other words, there was no 'stylized fact' (Kaldor, 1961) – no general relationship across time and space, from which 'universal' policy conclusions could be drawn.

This paper has two main goals. The first is to evaluate the veracity of Reinhart and Rogoff's 'stylised fact' as a credible representation of the debt-growth relationship across a range of countries – and thus as valuable evidence for economic policy formulation. Rather than assume that a single conclusion fits all countries, we use multilevel modelling to explore whether the relationship between those two economic indicators varies in its direction and intensity across the sampled countries – in that way introducing the 'situated messiness' that can reflect inter-country variations in the economic context within which the relationship between debt and growth is determined. We also explore whether the causal direction is the same – higher debt inhibiting growth – across all countries, or whether local circumstances is some generates a reverse causal link (if any). Should we accept the null hypothesis of no geographical variation, then the 'stylised fact' is a valid general conclusion and foundation for policy development. If, on the other hand (as proves to be the case), we reject that hypothesis because there are significant variations across countries – i.e. situated messiness – then we both challenge the utility of Reinhart and Rogoff's policy prescription and open up a field for research into why such geographical variation exists – something about which we make initial observations only. Indeed, there is only so much that can be achieved with the extensive variable-based approach we adopt here, and this needs to be supplemented by intensive case-based approaches (della Porta, 2008) that follows the detailed history of country trajectories. We have however shown unambiguously that there are different relations that need to be explained.

Varying relations between debt and growth

That second message is the focus of the discussion here. Our key argument is that Reinhart and Rogoff, in their attempts to come to a clear and 'clean' conclusion with strong policy implications, failed to exploit the potential of their data (as was also the case with Herndon et al., but in their case this was understandable because their paramount goal was to replicate Reinhart and Rogoff's findings).

What Reinhart and Rogoff failed to explore (and which therefore Herndon et al. only touched on) was variation across the 20 countries. Instead, their search for a single, simple conclusion meant that they did not consider the possibility that context might matter and things may vary by country. Their aim was 'to build the case for a stylised fact' (Herndon et al., 2013, p. 2), a single statement such as 'once the debt:GDP ratio exceeds 90, real growth will substantially decline'. Such hard and fast rules are often critiqued, particularly by economic geographers who believe that space and context matter. Clark (1998), for example, argued that a stylized fact is 'compromised by its reliance on a ready-made world' (p.73), a reliance which 'strips bare the complexity of life... stylized facts threaten the hard won work of the past twenty years aimed at integrating spatial heterogeneity into the theoretical core of economic geography' (p.74); Indeed, returning to the case of growth and debt, it may well be, for example, that:

- countries differ in their average GDP growth rates, whatever their debt levels, because of (possibly unique or at least particular to some countries only) characteristics that are outside the model; and
- countries differ in the intensity of the response to changes in the debt:GDP ratio – in some it may stimulate a steeper decline than others.

That one or more of these situations may characterise the countries analysed by Reinhart and Rogoff for the 1947-2009 period is readily appreciated by three simple graphs generated from the corrected data set made available by Herndon et al. The first (Figure 1) uses the Reinhart-Rogoff classification of country years into four groups according to the debt:GDP ratio. There is very substantial variation in the rate of real GDP growth around each group's mean value, strongly suggestive of factors other than the single 'independent variable' – the debt:GDP ratio – influencing the national rate of growth. This is further reflected by the R-squared value of just 4% found by Herndon et al. (2013, p. 22) in their regression of growth by these debt categories. The second (Figure 2) – similar to Herndon' et al.'s Figures 3 and 4 – shows that not only is the relationship between the two variables minimal compared to the overall variation of the data, but that there is also no apparent evidence that the slope becomes steeper once the debt:GDP ratio exceeds 90. (There are few points where the debt:GDP ratio exceeds 100 and it is likely that such points have excessive leverage on any computed relationship.) Finally, Figure 3 shows Reinhart and Rogoff's data for three countries only – Japan, New Zealand and United States – to illustrate our argument. For Japan, there is a general, although weak (given the wide scatter of points) negative relationship between the two variables; for New Zealand, there is if anything a positive relationship[1] – a high debt:GDP ratio is more likely to be associated with a high growth rate; and for the United States a steep negative relationship is very much a function of a single outlier (where the debt-to-growth ratio exceeds 100) only.

[Figs. 2 and 3 about here]

There are two reasons why consideration of this heterogeneity is important. The first is technical: failure to do so can lead to spurious relationships and non-linearities (such as Reinhart and Rogoff's 90% threshold) being estimated:

[1] The relationship for New Zealand would appear more strongly positive, were it not for two outliers. It so happens that one of these points is for 1951, the year of the Waterfront Strike (Brooking, 2004, p. 135). This point was the only one included in the dataset for New Zealand in Reinhart and Rogoff's original paper, and due to the weighting system used by Reinhart and Rogoff, this data point had a particularly strong influence on the apparent relationship found (Herndon, et al., 2013, p. 6), despite the low growth having very little to do with debt at all, and being unrepresentative of New Zealand in general. The need for detailed case analysis is clearly shown by this example.

> The commonly found 90% debt threshold is likely to be the outcome of empirical misspecification – a pooled instead of heterogeneous model – and subsequently a misinterpretation of the results, whereby it is assumed that pooled model estimates ... imply that a common non-linearity detected apples within all countries over time.
>
> (Eberhardt and Presbitero, 2013, 24-25)

Second, and perhaps more importantly, such heterogeneity is of genuine substantive interest, given that if it exists then policy devised on the basis of an overall stylised fact "may be seriously misguided" (Eberhardt and Presbitero, 2013, 24) in countries where that average rule does not apply. There are both theoretical and empirical reasons to expect differences between countries. Reinhart et al. (2003) argue that countries have different levels of debt tolerance, which depend in particular on their inflation history and strength of their state and financial institutions (see also Kraay and Nehru, 2006, Manasse and Roubini, 2009). Others have suggested that the effect of debt of growth depends either on its composition (Dell'Erba et al., 2013) or the specific production technologies in a given country (Eberhardt and Presbitero, 2013 p3, following Temple, 1999). This argument is supported empirically. In comparing potential tipping points across countries, Egert (2013) produces evidence for significant country specificity, finding only a few countries where there is any relationship at all; tipping points varied significantly both in the level of debt at which they occurred and the magnitude of the effect. Eberhardt and Presbitero (2013) found similar evidence of between-country heterogeneity, whilst Kourtellos et al. (2013) found that debt only affected growth in countries with undemocratic political regimes. The presence of heterogeneity does not necessarily mean that there is not additionally an overall effect when that heterogeneity is controlled out (e.g. see Chudik et al., 2013[2]), but clearly there is strong theoretical and empirical evidence to suggest that the possibility of varying relations between debt and growth is justified.

The question of reverse causality

An additional potential critique concerns the question of reverse causality. The associations found by Reinhart and Rogoff are used to suggest that debt levels have an impact on growth, but it is possible that that relationship operates, at least partly, in the other direction. There are certainly theoretical reasons for thinking that debt is likely to accumulate when growth is low (low growth means lower government revenue, meaning governments are forced into debt to maintain their welfare state and capital programmes). Reinhart and Rogoff (2009, xxxii) seem well aware of this causal pathway when they observed in earlier work that:

> Banking crises almost invariably lead to sharp declines in tax revenues as well as significant increases in government spending... On average, government debt rises by 86 percent during the three years following a banking crisis.

Yet in the later papers critiqued here they make no mention that this reversed relationship could be the cause of their observed correlation.

The possibility of such reverse causality is discussed by Dube (2013) in his reanalysis of Herndon et al.'s data. He argues that the apparent non-linearity of the association (a steep slope at low levels of debt and a shallower slope at higher levels – see Figure 2) is indicative of reverse causality, because one would expect steeper slopes to occur at high levels of debt because of tipping points. This conclusion is confirmed by impulse responses from his

[2] Chudik et al. (2013) find that, having controlled for heterogeneity, "the trajectory of the debt can have more important consequences for economic growth than the level of debt itself" (34), suggesting again that any effect depends on a country's specific fiscal policies for dealing with the debt that they have.

distributed lag models. Similarly, Irons and Bivens (2010) run Granger causality tests on data similar to Reinhart and Rogoff's (which hadn't been released when their paper was published.) Under a variety of numbers of lags, they find no evidence that debt 'Granger-causes' growth,[3] but there is evidence at all lags considered that growth Granger-causes debt.

Elsewhere, Reinhart and Rogoff (2010b) note the possibility of low growth (particularly in economic crises) leading to high debt, raising a question whether the association is bi-directional or whether all or most of it can be accounted for by a causal pathway from low growth to high debt. They argue from specific examples that the effect promoted as their 'stylised fact' operates, at least in part, with debt having an impact on growth; thus high debt over a long period was seen as a key factor in Greece's economic trouble in the late 2000s and early 2010s. Whilst a number of papers (Cecchetti et al., 2011, Checherita-Westphal and Rother, 2012, Furceri and Zdzienicka, 2012, Kumar and Woo, 2010) have used methods to control for reverse causality, and still find an apparent effect going from high debt to low growth, others find no such effect once reverse causality is properly accounted for(Panizza and Presbitero, 2012). Whilst these methods for dealing with reverse causality all have their flaws,[4] they raise the important point that the existence of some reverse causality does not mean that there could not be an effect in the original direction as well. The complexity of the possible causal pathways is explicated by the flow diagram of Figure 4. To justify the view that debt has a negative effect on growth, one must believe that the arrows pointing from debt towards growth are more substantively important than those going from growth to debt.

[3] Granger causality can be defined as when an unusual spike in X leads to a *later* spike in Y, tested using an autoregressive model including lagged values of both Y and X (Granger, 1969). Whilst this does not confirm causality (both increases could have been caused by a third omitted factor), it is indicative of it.

[4] Whilst many of the models that are used have their uses, none are without their flaws and no model can definitively prove causality or lack thereof. For example the use of instrumental variables (for example the General Method of Moments estimator) to address the endogeneity present in the model leads to only a small amount of the variance in the model being analysed, reducing both the statistical power of the model and the real world heterogeneity that we are often interested in (Deaton, 2010). Indeed, Panizza and Presbitero (2013) argue that these methods are unsuitable for datasets with relatively few cross-sectional units and so do not appropriately control for such reverse causality

However, as Dube (2013) points out, there is another, structural, reason for this reverse causality; the debt:GDP ratio includes GDP, so any growth will automatically reduce this ratio (and vice-versa).

[Figure 4 about here]

What none of these papers consider, however, is the potential heterogeneity in the extent of this reverse causality across time and space (periods and countries). Reinhart and Rogoff (2010b) may have found something interesting that occurred in Greece, but that does not necessarily imply that their stylized fact – that high debt leads to low growth – can be extended to a global rule. Similarly, those papers which find a bi-directional effect do not consider how these effects might vary from place to place.

Methodology

This paper's substantive contribution comes in two parts. The first ignores the issue of reverse causality and considers only how the association between debt and growth varies across countries. It deploys statistical methods which allow for a robust analysis of heterogeneity whilst maintaining some capacity to generalise where such generalisation is required or justified: multilevel models are perfectly suited for this purpose. The second part addresses the issue of reverse causality, and considers how that might itself vary between countries. In both cases, we used Reinhart and Rogoff's data, made available by Herndon et al. (2013).

Varying relations

The dataset used by Reinhart and Rogoff and Herndon et al. is hierarchically constructed: it comprises years as measurement occasions nested within countries and so can readily be analysed using multi-level modelling methods. These have characteristics that take into account major technical considerations regarding the data such as dependency within countries; data points within a country are more dependent on each other than occasions in different countries, making the assumption of homoscedasticity of standard linear regression modelling implausible.

Multilevel models account for differences between groups by partitioning variance between these hierarchical levels, so that dependence within groups is explicitly modelled. Moreover, the effects of variables can have their own variance such that slopes can be of different magnitudes for different groups of observations (countries in this case); these different magnitudes are shrunk towards a common mean according to the reliability of that country's estimate, making the results very robust (Snijders and Bosker, 2012, p.62). Additionally, the variance at the lowest level can be modelled as a function of covariates. Thus, 'the specifics of people and places are retained in a model, which still has a capacity for generalisation' (Jones, 2005, p.255).

A simple version of our model is thus as follows:

$$Growth_{ij} = \beta_{0j} + \beta_{1j}Debt_{ij} + e_{0ij} + e_{1ij}Debt_{ij}$$

$$\beta_{0j} = \beta_0 + u_{0j}$$

$$\beta_{1j} = \beta_1 + u_{1j}$$

These equations – one at the micro (occasion) level and the other two at the macro (country) level – combine to form:

$$Growth_{ij} = \beta_0 + \beta_1 Debt_{ij} + [u_{0j} + u_{1j}Debt_{ij} + e_{0ij} + e_{1ij}Debt_{ij}]$$

Where $Growth_{ij}$ is the year per cent change in real GDP, and $Debt_{ij}$ is the debt to GDP ratio of country j in year i. The β_0 term is the average intercept, which differs for countries by u_{0j}, and β_1 is the average effect of the debt:GDP ratio, which differs across countries by u_{1j}. At level 1, the variance is allowed to vary as a function of debt, via the additional residual term e_{1ij}. This is both substantively interesting and technically important so as to avoid variance being assigned to the higher level erroneously (Rasbash et al., 2009). At both level 1 and 2, these variances are assumed to be bivariate Normal, with the intercept variance, slope variance and covariance at both levels estimated from the data, such that:

$$\begin{bmatrix} u_{0j} \\ u_{1j} \end{bmatrix} \sim N\left(0, \begin{bmatrix} \sigma_{u0}^2 & \\ \sigma_{u0u1} & \sigma_{u1}^2 \end{bmatrix}\right)$$

$$\begin{bmatrix} e_{0j} \\ e_{1j} \end{bmatrix} \sim N\left(0, \begin{bmatrix} \sigma_{e0}^2 & \\ \sigma_{e0e1} & \sigma_{e1}^2 \end{bmatrix}\right)$$

In our models, within and between effects were modelled as separate effects, which were allowed to differ by including the country mean of debt (Bell and Jones, 2014a). This negates the need to perform a Hausman test (Hausman, 1978) often used to justify the use of fixed effects models instead of the multilevel models used here. The model is thus elaborated to become:

$$Growth_{ij} = \beta_0 + \beta_1(Debt_{ij} - \overline{Debt}_j) + \beta_2\overline{Debt}_j + [u_{0j} + u_{1j}(Debt_{ij} - \overline{Debt}_j) + e_{0ij} + e_{1ij}(Debt_{ij} - \overline{Debt}_j)]$$

Where β_1 is the within (longitudinal) effect, and β_2 the between (cross-sectional) effect, of debt. Whilst this within-between separation led to an improvement in the fit of some of the models analysed here, according to the Deviance Information Criterion (DIC, see Spiegelhalter et al., 2002), it did not change any of the substantive conclusions.

A number of questions are raised regarding how best to formulate such a model; what form might the relationship take? Looking at the raw data suggests that there may be non-linearities, with the relationship being strongest at low levels of debt (see Figures 2 and 3). Rather than analyse the ‘raw’ data, Reinhart and Rogoff collapsed the debt:GDP ratio variable into just four categories – country:years with ratios of: less than 30, between 30 and 59, between 60 and 89, and 90 or more. (We assume that the boundaries were determined after empirical explorations since no theoretical rationale was offered for them – nor can one be readily constructed: that the ‘debt burden cliff’ came at a ratio of 90 was an outcome of the chosen classification.) However, if we believe Reinhart and Rogoff's implication that there is a threshold of 90 per cent debt beyond which growth should decline, it would again make sense to model the debt variable in a non-linear way. Thus, we consider both models which allow for quadratic functions of debt on growth, and others which include a dummy variable for country-years with debt ratios exceeding 90, alongside Reinhart and Rogoff’s other three categories, instead of a linear trend.

A further question is whether to include a time trend in the models. Reinhart and Rogoff do not, and thus effectively only consider the raw association between the two variables. However, it could be that the association is in part the result of long-run changes over time, in debt ratios, growth rates, or both, which are not related to the growth-debt causal pathway. We also want to test whether that trend is global, across all countries, or more local, varying between countries – if the latter occurs, then those local trends need to themselves be controlled for. This is done by comparing the model’s fit where the year effect is (a) assumed fixed across all countries, and (b) allowed to vary between countries. Again, the linearity of this effect needs to be tested by the inclusion of polynomial terms, and the combined model becomes (assuming all effects are linear and local time trends are being modelled):

$$Growth_{ij} = \beta_0 + \beta_1(Debt_{ij} - \overline{Debt_j}) + \beta_2\overline{Debt_j} + \beta_4 Year_{ij} + \left[u_{0j} + u_{1j}(Debt_{ij} - \overline{Debt_j}\right) + u_{2j}(Year_{ij}) + e_{0ij} + e_{1ij}(Debt_{ij} - \overline{Debt_j}) + e_{2ij}(Year_{ij})]$$

This formulation enables an assessment of the effect of debt levels on growth, controlling for any exogenous trends that may be affecting the growth variable over time. The raw data suggest that there was a general decline in growth over time (see Figure 5a), which would be accounted for by this term, leaving any additional effect of debt over this to be determined. We report results of our models both with and without the year term included, so that the latter can be compared to Reinhart and Rogoff's findings.

We fitted the following multilevel models:

1. A model with just linear (within and between) covariates for debt, with the variance partitioned between countries and occasions (random intercepts model);
2. As model 1, but with the within effect allowed to vary at both levels 1 and 2;
3. As model 2, but with a linear year term included in the fixed part;
4. As model 3, but with the year term allowed to vary at both levels;
5. A model with dummy terms for each of the categories used by Reinhart and Rogoff (random intercepts model)
6. As model 5, but with the effect of each dummy variable allowed to vary at both levels 1 and 2;
7. As model 6, but with a linear year term included in the fixed part of the model
8. As model 7, but with the year term allowed to vary at both years.

We additionally tested for the significance of a quadratic fixed part term for both the year and the debt term, but these were found to be statistically insignificant.

All models were fitted using MLwiN version 2.27 (Rasbash et al., 2013) using Monte Carlo Markov Chain (MCMC) Gibbs sampling methods with non-informative priors defined by Iterative Generalised Least Squared (IGLS) maximum likelihood (ML) estimates of the same models, where possible (Browne, 2009).[5] MCMC methods are superior to standard ML estimation when there is a small number of groups into which the observations are nested (Bell and Jones, 2014b, Stegmueller, 2013). Since Reinhart and Rogoff's data include just 20 countries, we can expect an improvement in the accuracy of both point estimates and standard errors from using MCMC rather than ML estimation. We ran the MCMC runs for 20,000 iterations, plus a 1,000 iteration discarded initial burn-in, which was sufficient to achieve a reasonable effective sample size without any trending in the chains (assessed by visual inspection).

Tests of reverse causality

So far we have assumed that high debt is a cause of low growth, and not the other way around. However, as Figure 4 shows, this assumption is not necessarily appropriate. Indeed, one would expect low levels of growth to force the government to spend more in an attempt to stimulate the economy, as well as needing debt to fund existing public expenditure with reduced revenue from taxes. The question is to what extent the relationships that we have found are the result of debt causing growth, and what extent they are the result of the opposite causal effect.

In order to do this, we use a distributed lag model, which assesses the effect of a supposed increase in debt on growth. If debt in general causes growth, we would expect to see the rise in debt occurring before a rise in growth. Conversely, if it were more often the case that

[5] For some of the more complex models, it was not possible to fit the models using ML or restricted ML. In these cases, plausible values were used as non-informative priors, obtained from MCMC estimates of similar but simpler models.

growth causes debt, we would expect to find the rise in debt occurring after the rise in growth. Dube (2013) uses a distributed lag model in this way, using Reinhart and Rogoff's data, and finds that the effects of lags on debt were smaller than the effects of leads. In other words, a supposed increase in debt seems to have an effect on growth in the past, but not growth in the future. This is not logical, and suggests that primarily it is growth that is causing debt and not the other way around.

Dube's model assumed, however, that the causal direction was consistent across countries. It may be that in some countries the effect of growth on debt is much greater than the effect of debt on growth (for example due to the nature of their financial institutions). We have therefore developed a multilevel version of the distributed lag model. This is a two-level model (occasions nested in countries); the first differences of three lags, the raw variable and three leads of debt are included in the model, and these effects are allowed to vary at level 2.[6]

The model is thus specified as follows:

$$
\begin{aligned}
Growth_{ij} = \beta_0 &+ \beta_1(Debt_{i-3j}) + \beta_2(\Delta Debt_{i-2j}) + \beta_3(\Delta Debt_{i-1j}) + \beta_4(\Delta Debt_{ij}) \\
&+ \beta_5(\Delta Debt_{i+1j}) + \beta_6(\Delta Debt_{i+2j}) + \beta_7(\Delta Debt_{i+3j}) + \beta_8 Year_{ij} + [u_{0j} \\
&+ u_{1j}(Debt_{i-3j}) + u_{2j}(\Delta Debt_{i-2j}) + u_{3j}(\Delta Debt_{i-1j}) + u_{4j}(\Delta Debt_{ij}) \\
&+ u_{5j}(\Delta Debt_{i+1j}) + u_{6j}(\Delta Debt_{i+2j}) + u_{7j}(\Delta Debt_{i+3j}) + u_{8j}(Year_{ij}) \\
&+ e_{0ij}]
\end{aligned}
$$

$$
\begin{aligned}
&u_{0j} \sim N(0, \sigma^2_{u0}),\ u_{1j} \sim N(0, \sigma^2_{u1}),\ u_{2j} \sim N(0, \sigma^2_{u2}),\ u_{3j} \sim N(0, \sigma^2_{u3}),\ u_{4j} \sim N(0, \sigma^2_{u4}), \\
&u_{5j} \sim N(0, \sigma^2_{u5}),\ u_{6j} \sim N(0, \sigma^2_{u6}),\ u_{7j} \sim N(0, \sigma^2_{u7}),\ u_{8j} \sim N(0, \sigma^2_{u8}),\ e_{0ij} \sim N(0, \sigma^2_{e0}).
\end{aligned}
$$

[6] Due to the complexity of such models, it was not possible to fit them allowing for the effects to vary at level 1. It was also not possible to include covariances between the random effects for each of the lags. More data would be required for such models to be feasible.

As with the previous models, this was estimated using MCMC estimation in MLwiN, run through Stata using the runmlwin command (Leckie and Charlton, 2013); the do-file for implementing this can be found in this article's online appendix. The model was run for 20000 iterations following a 2000 iteration burn-in, using IGLS estimates as starting values where possible. We ran this model with and without a control included for year (although the results were broadly similar in either case, so here we only report the results where year is controlled in both the fixed and random parts of the model).

Results

Varying relations

Figure 5 shows the relationships among the three variables in the dataset – debt, growth and time – across all countries in the sample. Over time, growth is highly erratic (5a), compared to debt which changes much more slowly (5b). This means that when using debt to explain growth, we will only be able to account for a small proportion of the variance in growth. Additionally, Figure 5c shows that the number of occasions when the debt:GDP ratio is over 90 per cent is fairly limited, although there is some evidence of occasions where growth has been low in such situations, most notably in Japan, UK and Australia.

[Fig. 5 (a-c) about here]

Model 1 is the simplest model (apart from the null model 0) presented here, with only a linear effect of debt (both within and between) included in the model. The parameter estimates (see Table 1) show that the average effect is significantly negative. However, when (in model 2) the within effect of debt is allowed to vary between countries, variation between countries in the size of the effect emerges, as shown in Figure 6a. Some countries have a significant downward trend, but others do not – and some (for example New Zealand) have an apparent positive trend. The estimates suggest that countries such as the UK and Australia have had effectively no effect of debt on growth at all. Certainly, Figure 6a provides little evidence for an overall rule – such as Reinhart and Rogoff's 'stylised fact' – that can be applied across the board.

The story is somewhat different when time is controlled as a simple fixed effect in model 3. Here, the average within effect of debt is smaller than that in model 2 but appears to be more consistent across countries, whilst the estimated between-country variance in the slope is statistically insignificant (Figure 6b). This is in line with Reinhart and Rogoff's findings and implies that the 'stylised fact' that they argue for may be valid – there are no significant differences between countries in the size of the effect of debt when a global time trend is controlled for. However we additionally tested to see if the variation in the data is better accounted for by local country trends by allowing the effect of year to vary. Doing so improved the DIC significantly suggesting that there are indeed important differences in the overall trend of countries' growth over time.

When the effect of Year was allowed to vary (model 4), the significance of the average effect of debt disappeared. The increased homogeneity of the trends identified in model 3 was the result of controlling for a global time trend that was not representative of trends in growth in any individual country. However, model 4 does show that the effect of debt is itself country dependent (Figure 6c). Some countries (in particular Japan, Ireland and the US) appear to have trends which are in line with Reinhart and Rogoff's hypothesis. But others appear to have effects that, if anything, operate in the opposite direction, and this includes Greece, cited by Reinhart and Rogoff (2010b) as a prime example where an increase in debt has caused a decrease in growth.

[Fig. 6 (a-c) about here]

[Table 1 about here]

Models 5-8 consider the situation when the debt variable is reduced to Reinhart and Rogoff's four categories. The results are broadly similar to those for models 1-4 (see Figure 7a-c and table 2). Controlling for year as a fixed effect reduces the size of the mean effect, but those effects remain significant, particularly for the over-90 per cent debt category. However when the year effect is allowed to vary, the effects of all debt categories become statistically insignificant.

[Fig. 7 (a-c) about here]

[Table 2 about here]

An additional finding of these models is in the variance of the level 1 residuals. Figure 8 and 9 show that, for both continuous and categorical debt, high levels of debt are associated with significantly more variance between occasions than the lower debt categories, a finding represented by a significant increase in the level 1 variance over time (the level 2 variance, in contrast is not found to be related to debt). This suggests that the growth rate is more erratic when countries have a high level of debt – some have very high and some very low growth years when experiencing a high debt level. An example of this is New Zealand in 1951, when the Waterfront strike (Brooking, 2004, p. 135) brought the country into recession in a year that happened to also have high levels of debt. Whilst this acted only to heavily skew the results found by Reinhart and Rogoff (Herndon et al., 2013, p. 6), here we are able to model it in a substantively interesting way whilst still being able to model general trends when they are present. Highly erratic growth may still be considered undesirable (especially given that volatility could itself have a negative effect on growth – see Ramey and Ramey 1995), meaning that incurring high levels of debt would be considered unwise. However, clearly the story remains rather more complex than that presented by Reinhart and Rogoff.

[Figs. 8 and 9 about here]

Reverse Causality

The results from our multilevel distributed lag model are shown as impulse responses, separately for each country, in Figure 10 (the parameter estimates are in Table 3). Here, the horizontal axis represents the time before a supposed change in level of debt (with leads to the left of zero and lags to the right). In general the biggest negative effects of debt on growth appear to be in the leads (on the left-hand sides of the graphs) rather than the lags. This suggests, in line with Dube's (2013) findings, that the negative relationships we found in the first part of our analysis are predominantly the result of the effect of growth on debt, rather than the effect of debt on growth. What is new here is that this varies to some extent between countries. In some (such as the United States) this pattern is very strong, suggesting an effect that is predominantly in the growth to debt direction; in others (such as Ireland) this is much less clear, suggesting that the direction of causation may be at least in part in the direction suggested by Reinhart and Rogoff. Finally, in line with our previous results, in many countries (such as Norway and Canada) there is little evidence of any relationship between growth and debt at any lag or lead.

[Fig. 10 about here]

Whilst the purpose of this paper is mainly to reveal, rather than explain, the messiness in the relationship between growth and debt, it is worth at this point speculating as to possible causes of this heterogeneity. A closer look at the data reveals that it is mainly the post-war years that are driving the big relationship found in the United States – worth noting but hardly the basis for forming a causal understanding in typical macroeconomic scenarios. In that case, the reverse causality is clear: high debts following the Second World War lead to a subsequent period of low growth. In Japan, by contrast, the higher levels of debt are in recent years (including the financial crisis). However that the relationship in Japan does not hold when the period effect is controlled in the highest category (see Figure 7c) suggests that the rise of debt and the decline of growth are co-existent trends rather than there being a particular causal link. Perhaps the best evidence for a causal effect of debt on growth comes from Ireland; however it is noteworthy that it is alone amongst European Union countries in experiencing this negative relationship, with most showing if anything a positive relationship between growth and debt when time is fully controlled (Figure 6c). This suggests that membership to the EU could provide a cushion to any effect that debt may have on growth (or, vice versa). A number of countries such as Canada appear to have little relation between growth and debt, perhaps because of the importance of the primary sector in its economy (which could be less susceptible to debt uncertainty than other sectors).

Conclusions

This paper has made two key points. The first is that there is variation in the relationship between debt and GDP across countries. There have been significant differences both between and within countries which make it clear that the association between debt and growth is more complex than Reinhart and Rogoff's stylised fact and, when a time trend is appropriately controlled for, any average effect of debt on growth becomes insignificant. This conclusion is both substantively important to the study of the relationship and its policy implications and methodologically important for all researchers considering hierarchically nested data. Substantively these findings clearly indicate that, with regards to the effect of debt on growth, an argument and implied policy prescription based on a 'stylised fact' alone is much too simplistic and general to be particularly relevant from a policy perspective. Methodologically, we argue that testing how effects vary with context as we have here should be as routine as testing other statistical modelling assumptions, across economics and the social sciences.

The second point is that, whilst the relationship between debt and growth may appear significant in some modelling scenarios, evidence from our multilevel distributed lag model suggests that for most countries the direction of causation is predominantly in the opposite direction to that mooted by Reinhart and Rogoff. However this too is subject to variation between countries with some, such as Ireland, exhibiting a less clear causal direction.

The analyses reported here are somewhat limited by the small size of the dataset (both in terms of sample size and lack of potential control variables) and the weakness of the observed effects (in terms of the proportion of the variance explained). It is difficult to estimate such complex models with minimal data, which may account for some of the reported statistically insignificant results. But the nature of our conclusions remains substantively important: the relationship between debt and growth varies across countries and has limited explanatory power, with debt accounting for only a very small proportion of the variance at both levels. As such, the importance of debt for budgetary policies should not be exaggerated; much depends on time *and place*.

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Figure 1: The relationship between debt and economic growth, using the corrected Reinhart-Rogoff data, with values on the X-axis grouped into four groups (<30; 30-59; 60-89; 90<). A linear regression line between the two variables is also shown.

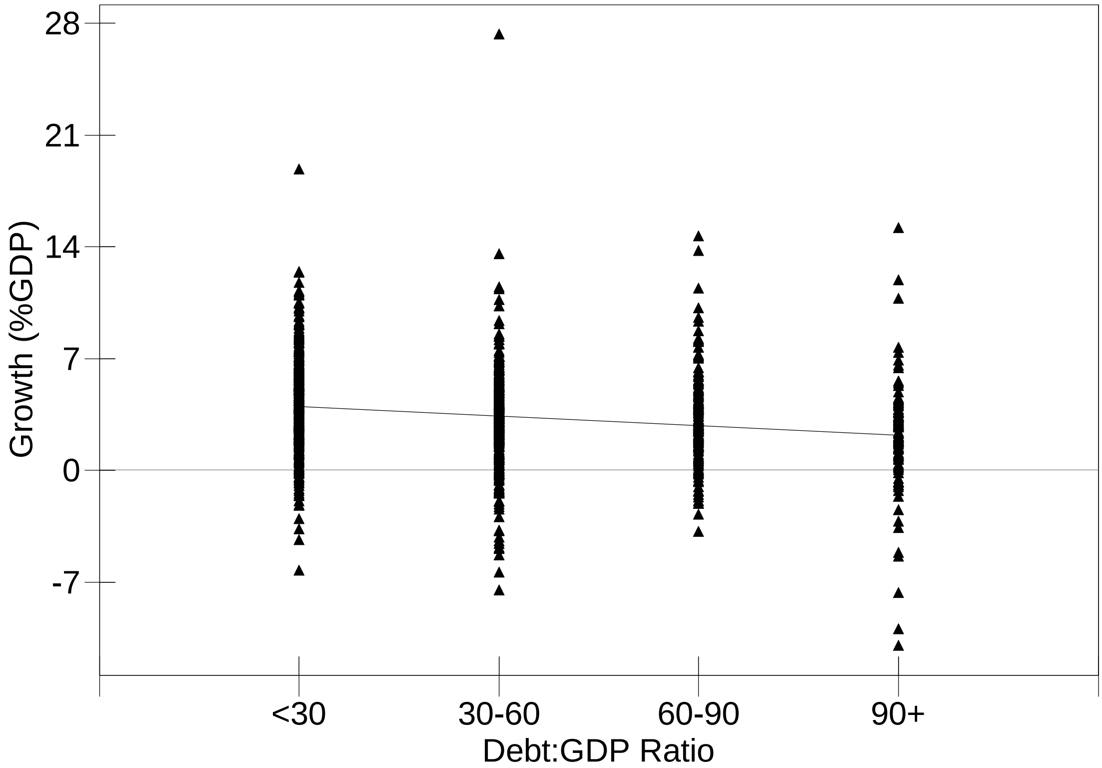


Figure 2: (a) Raw data of debt against growth, with a fitted smoothed line, and (b) the same smoothed line without the raw data. The model was fit with a Generalised Additive Model using the mgcv package in R (Wood, 2006). Dotted lines are 95% confidence bounds. These figures replicate figures 3 and 4 in Herndon et al (2013).

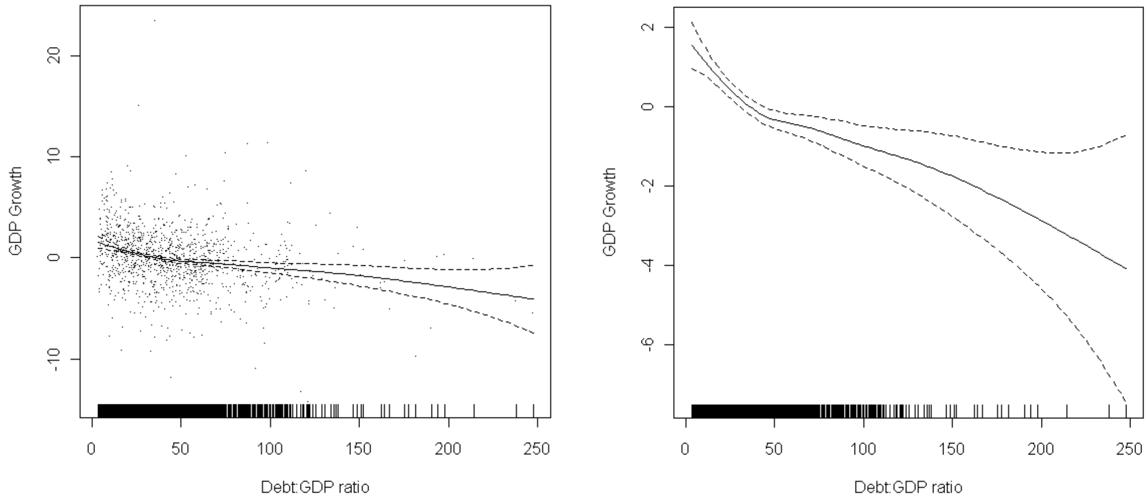


Figure 3. The relationship between the debt:GDP ratio and the real rate of GDP growth in Japan, New Zealand and the United States.

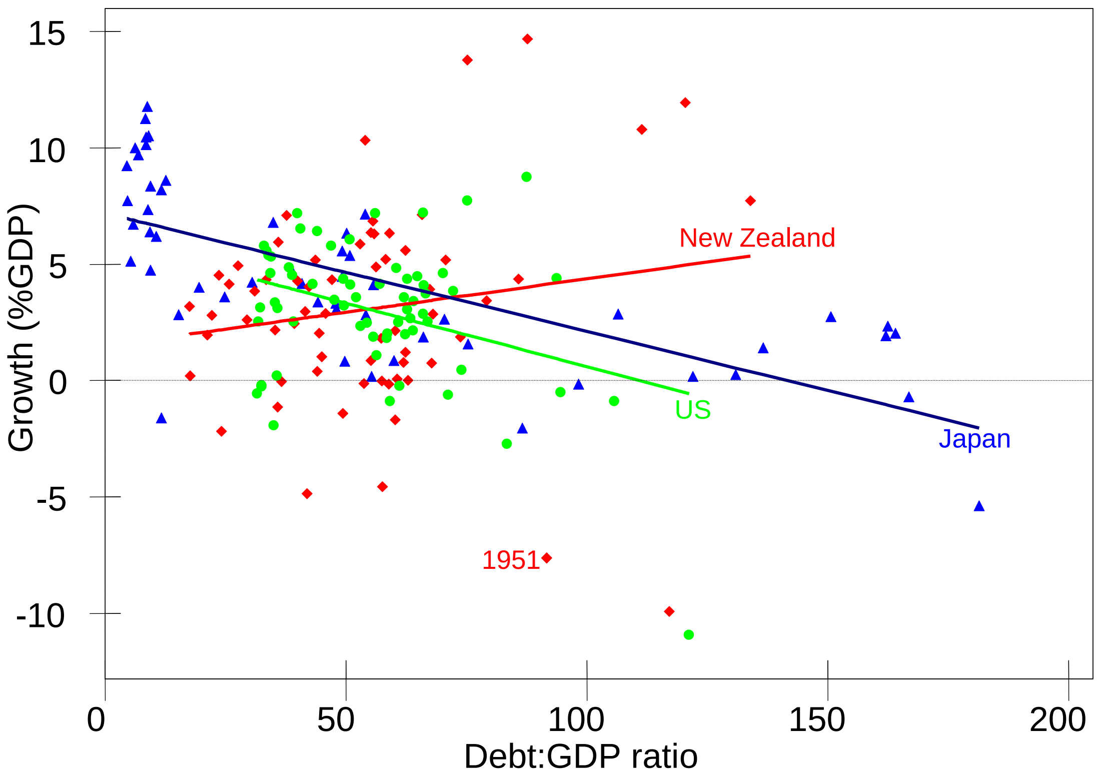


Figure 4: flow diagram of possible causal pathways between growth and debt. Based on Reinhart and Rogoff 2011 and Irons and Biven 2010.

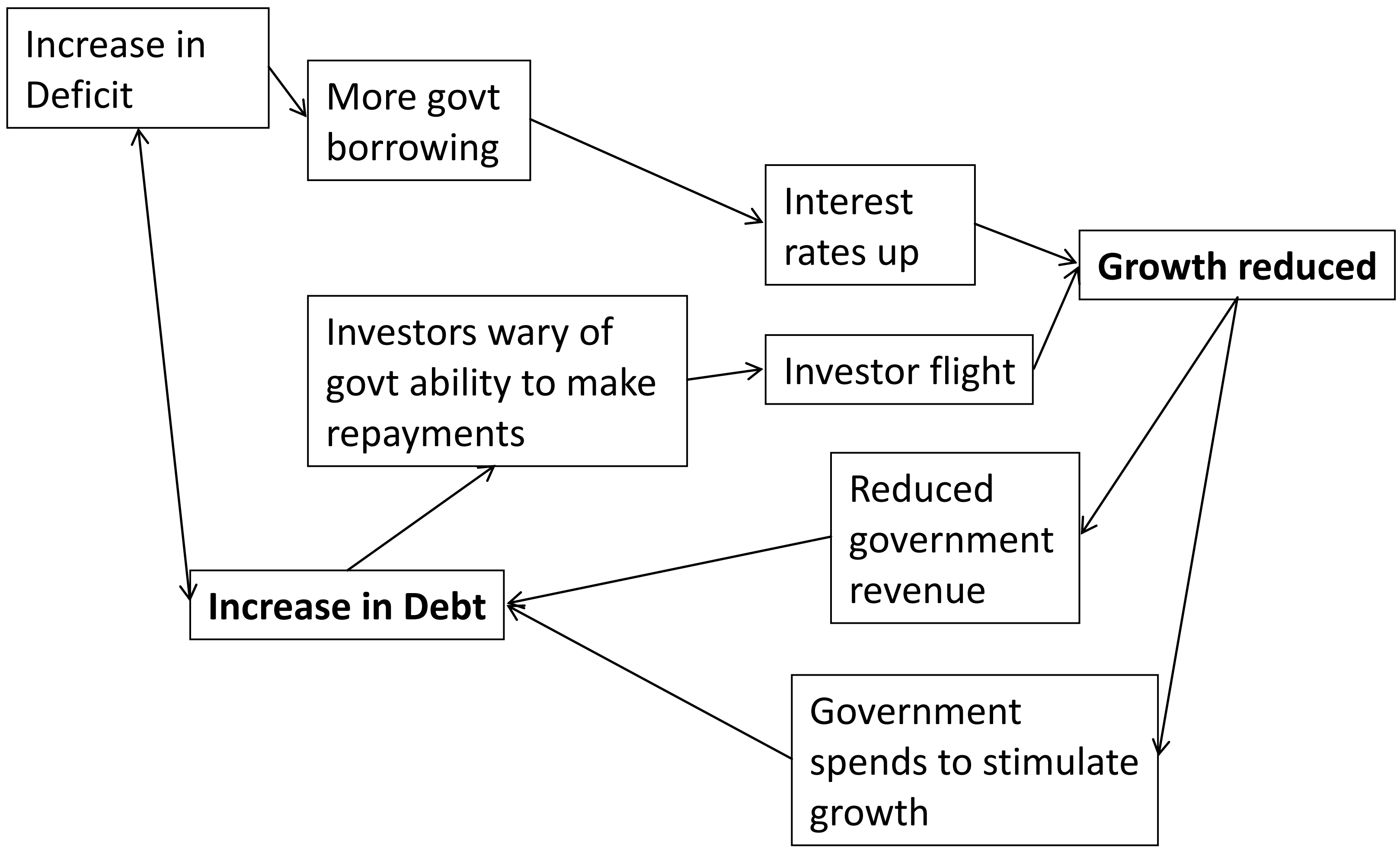


Figure 5: Graphs showing the relationships between debt, growth and time (raw data). (a) How growth varies over time for each country; (b) how debt varies over time, for each country; (c) the relationship between debt and growth.

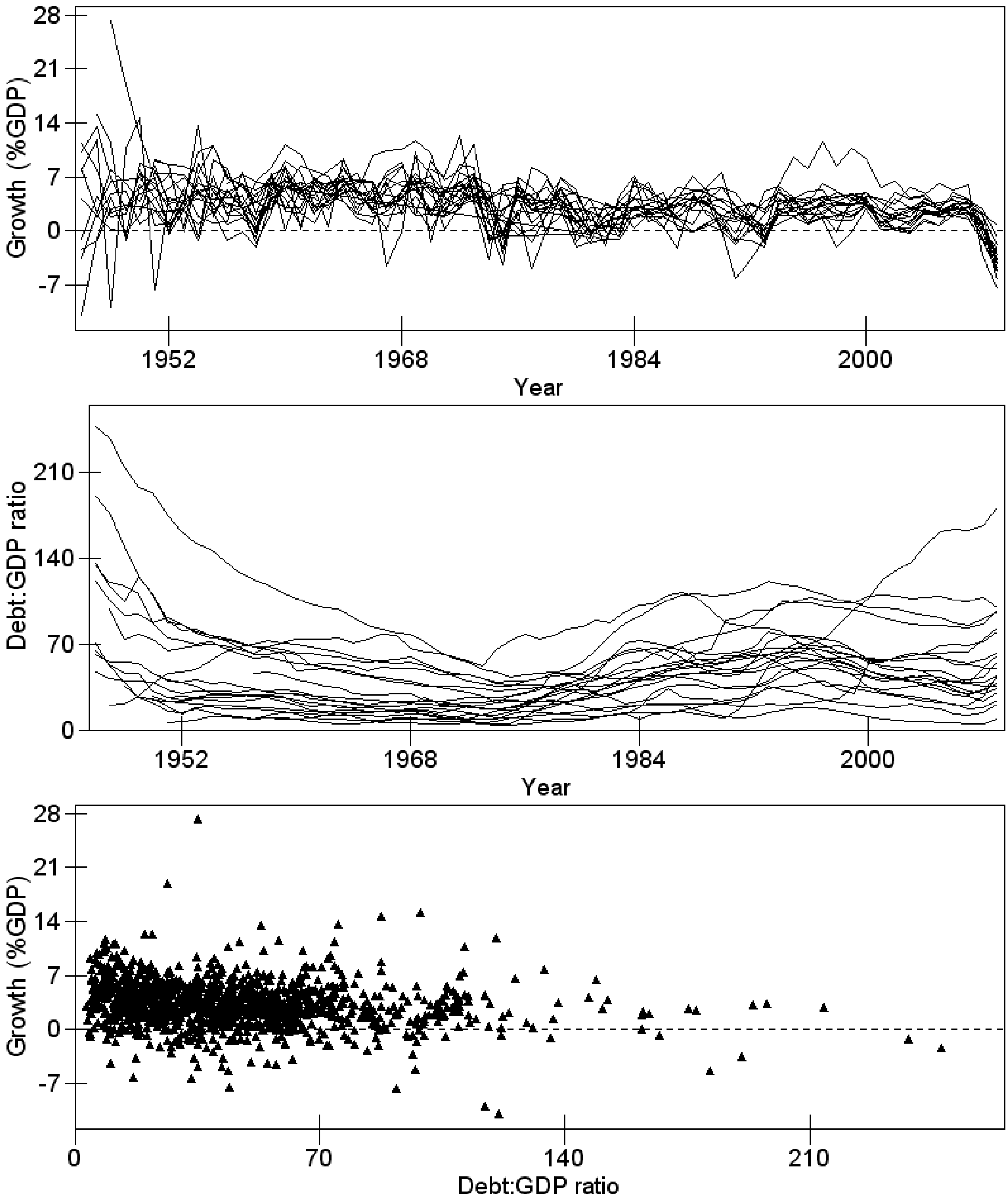


Figure 6a - Predicted growth as a function of (continuous) debt, from model 2, where Year is uncontrolled. Notable countries are highlighted

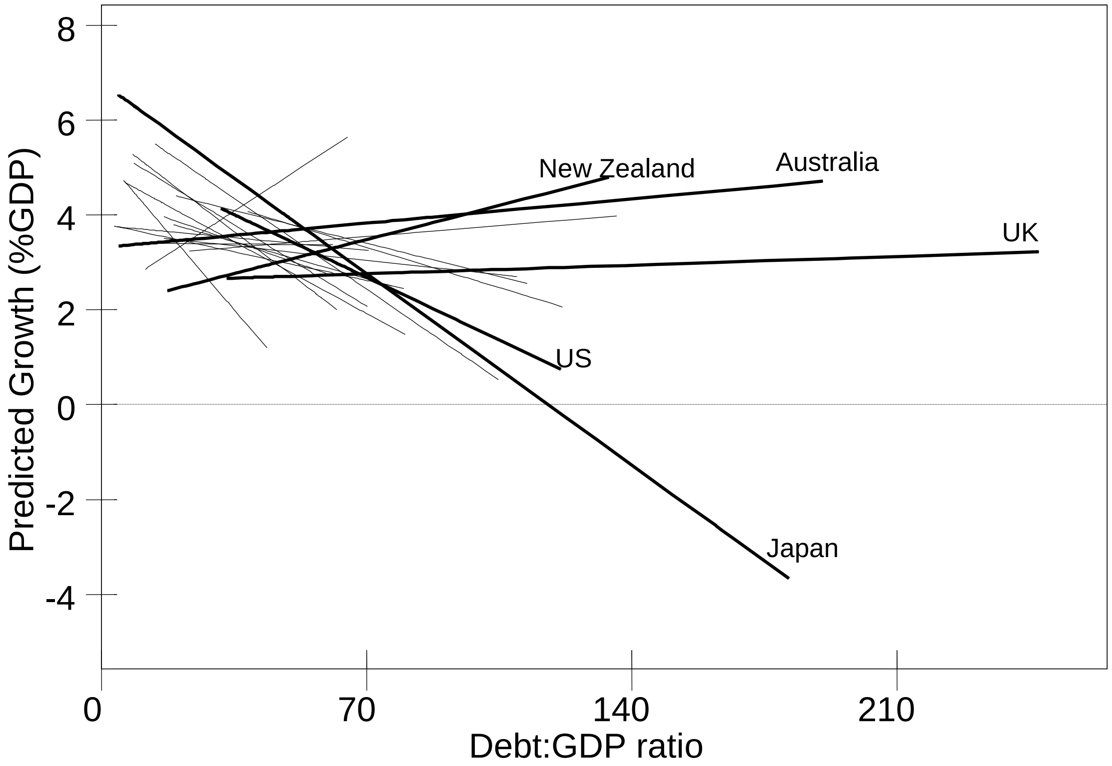


Figure 6b - Predicted growth as a function of (continuous) debt, from model 3, where Year is controlled in the fixed part of the model only. Notable countries are highlighted

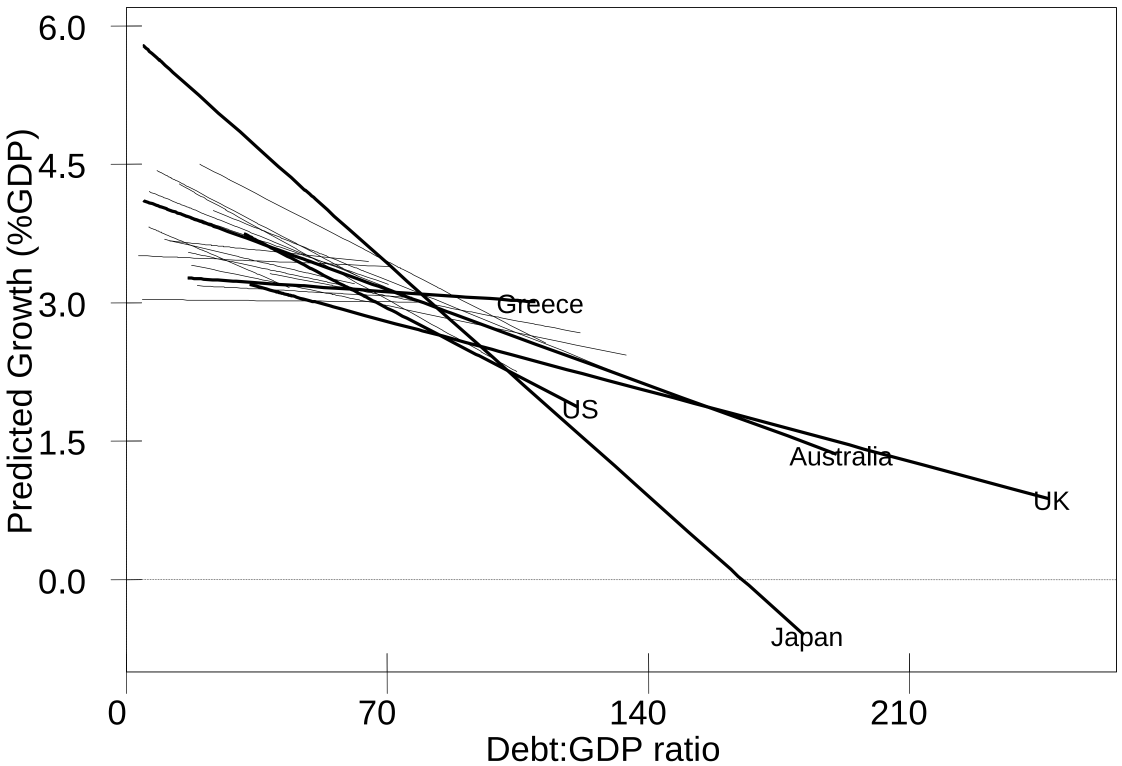
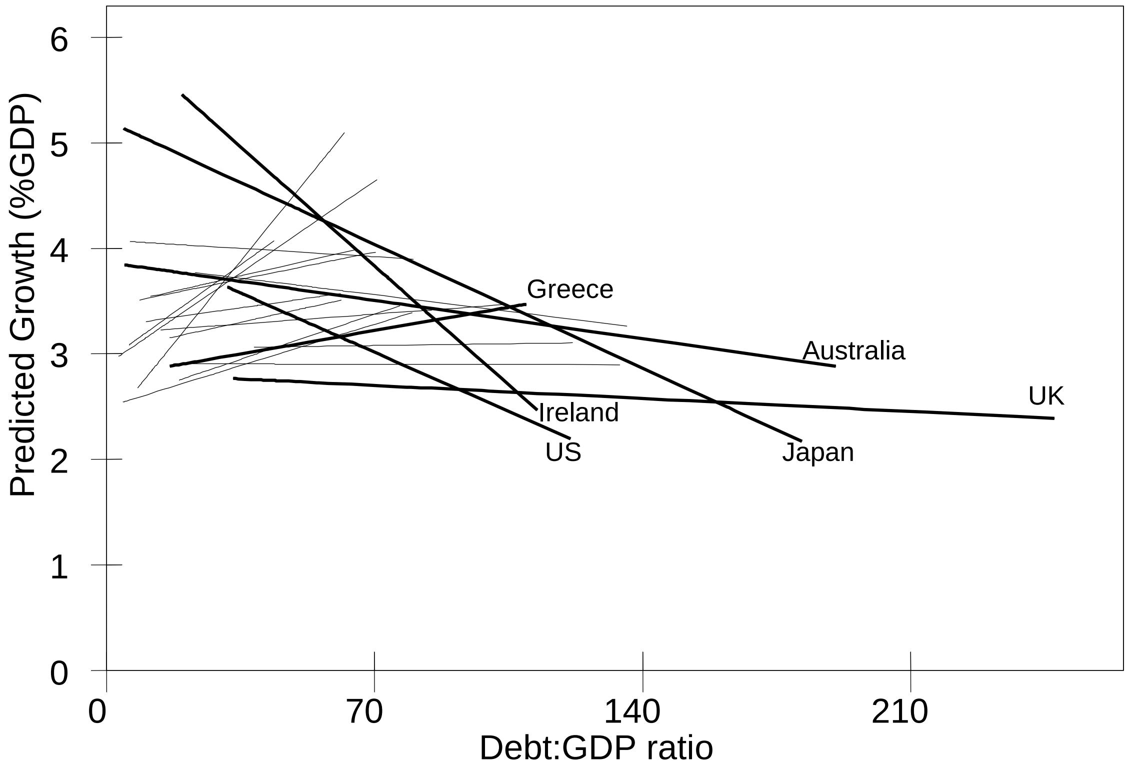


Figure 6c - Predicted growth as a function of (continuous) debt, from model 4, where Year is controlled in both the fixed and the random parts of the model. Notable countries are highlighted. Panel 2 shows EU countries only.



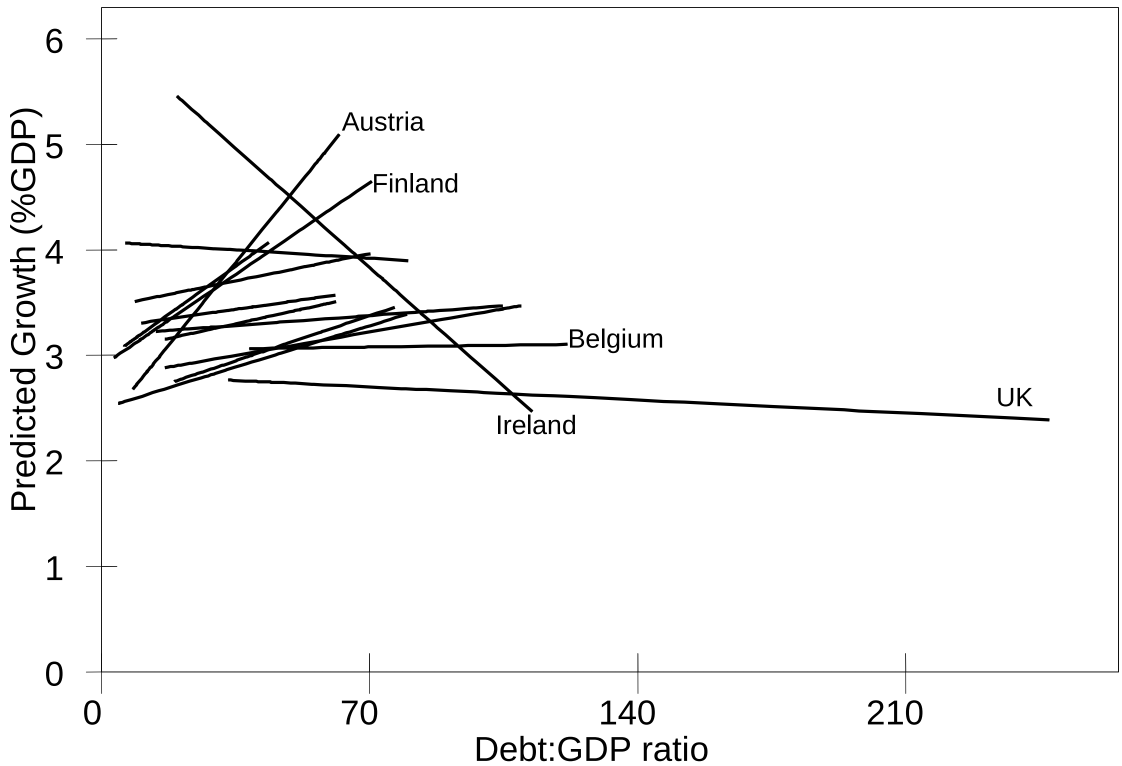


Figure 7a - Predicted growth as a function of (categorical) debt from model 6, where Year is uncontrolled. Notable countries are highlighted

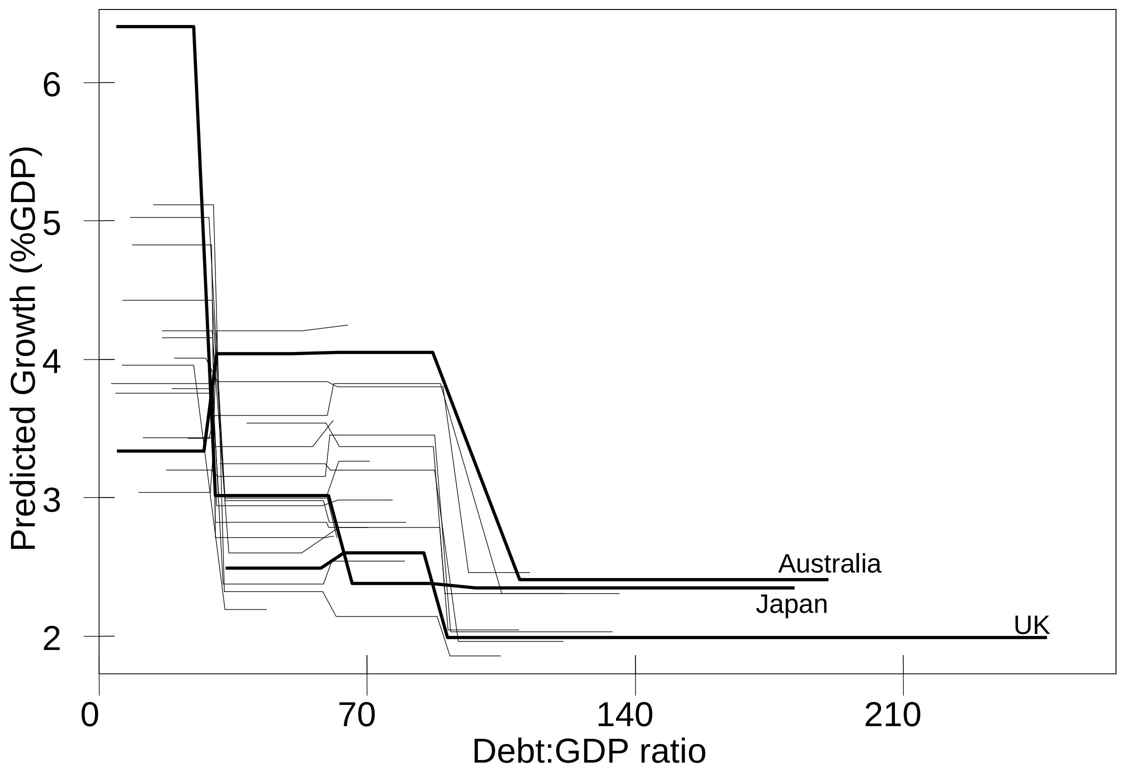


Figure 7b Predicted growth as a function of (categorical) debt from model 7, where Year is controlled in both the fixed part of the model only. Notable countries are highlighted

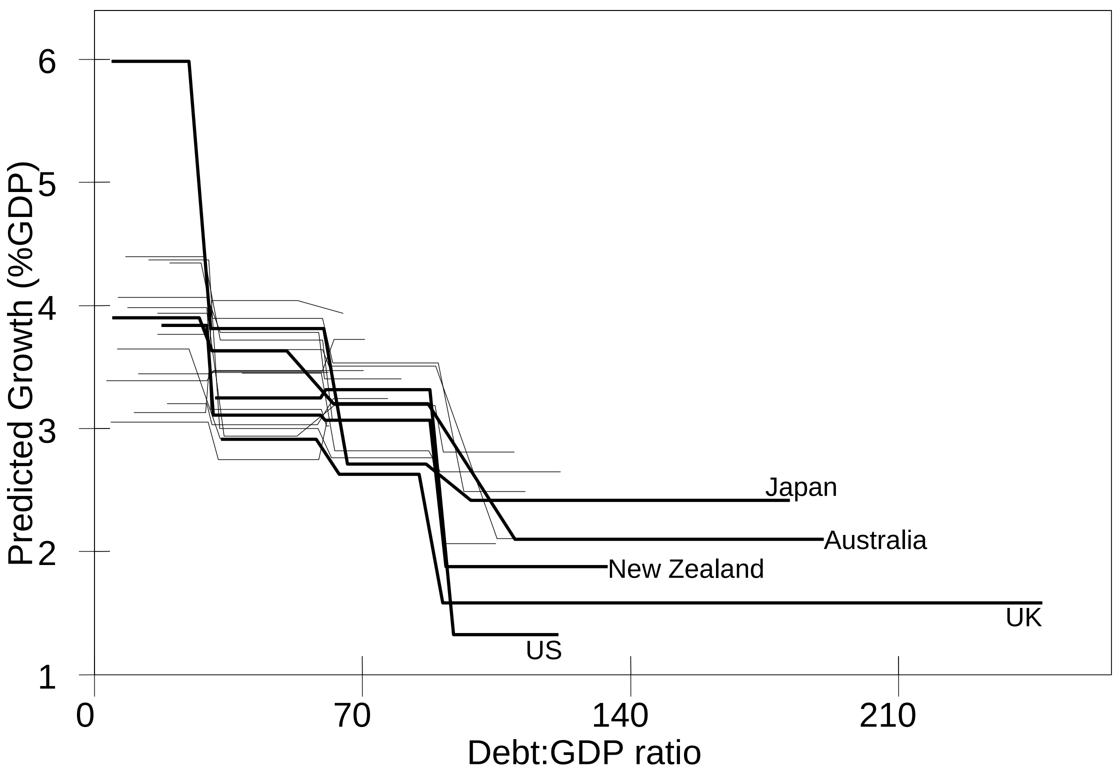
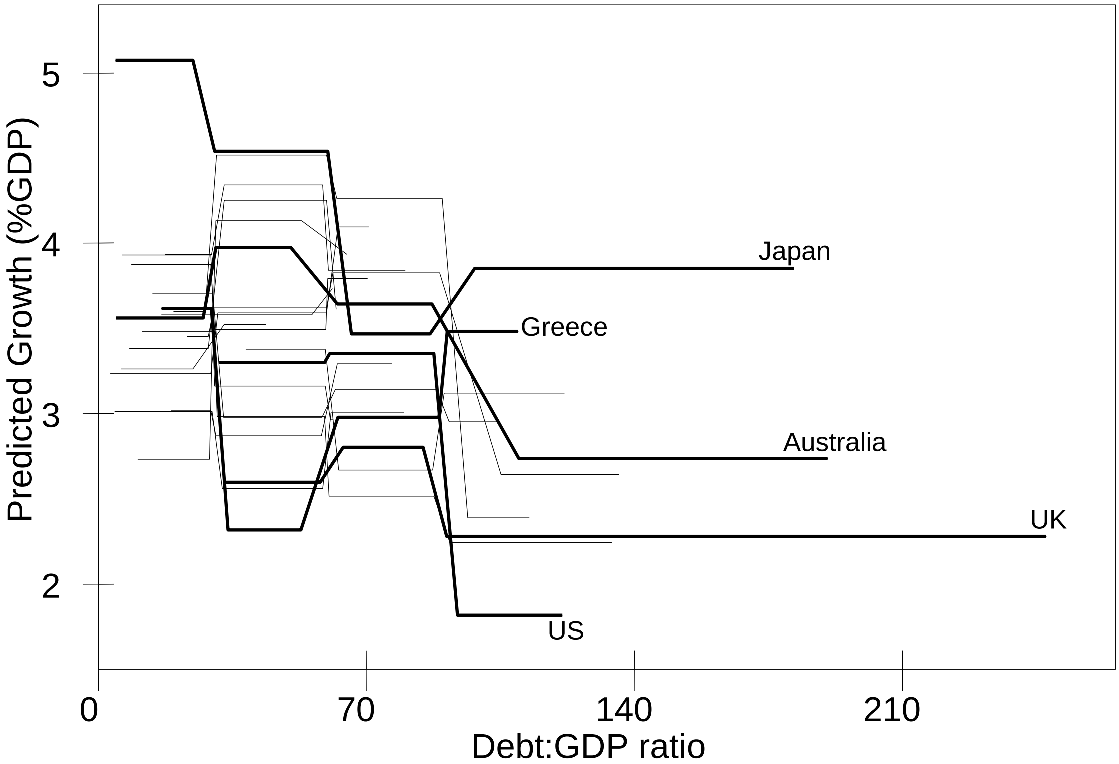


Figure 7c - Predicted growth as a function of (categorical) debt from model 8, where Year is controlled in both the fixed and random parts of the model. Notable countries are highlighted. Panel 2 shows EU countries only.



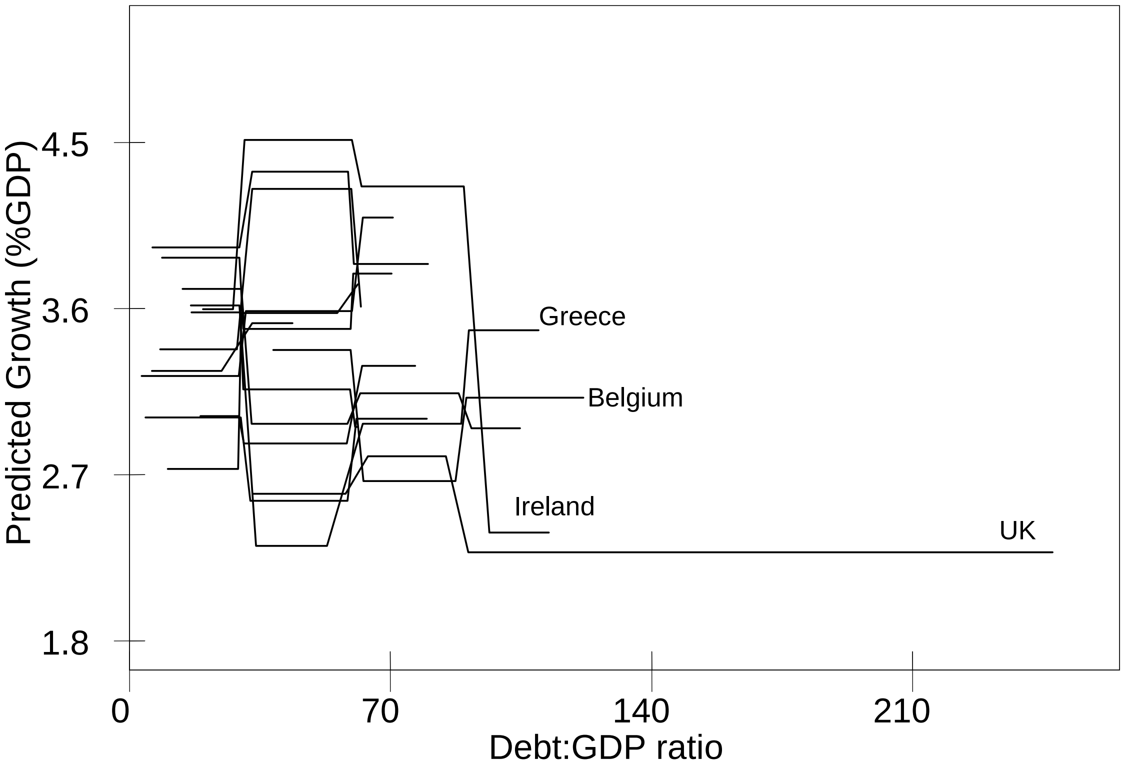


Figure 8 Variance Functions, from model 3, at level 1. With 95% confidence intervals

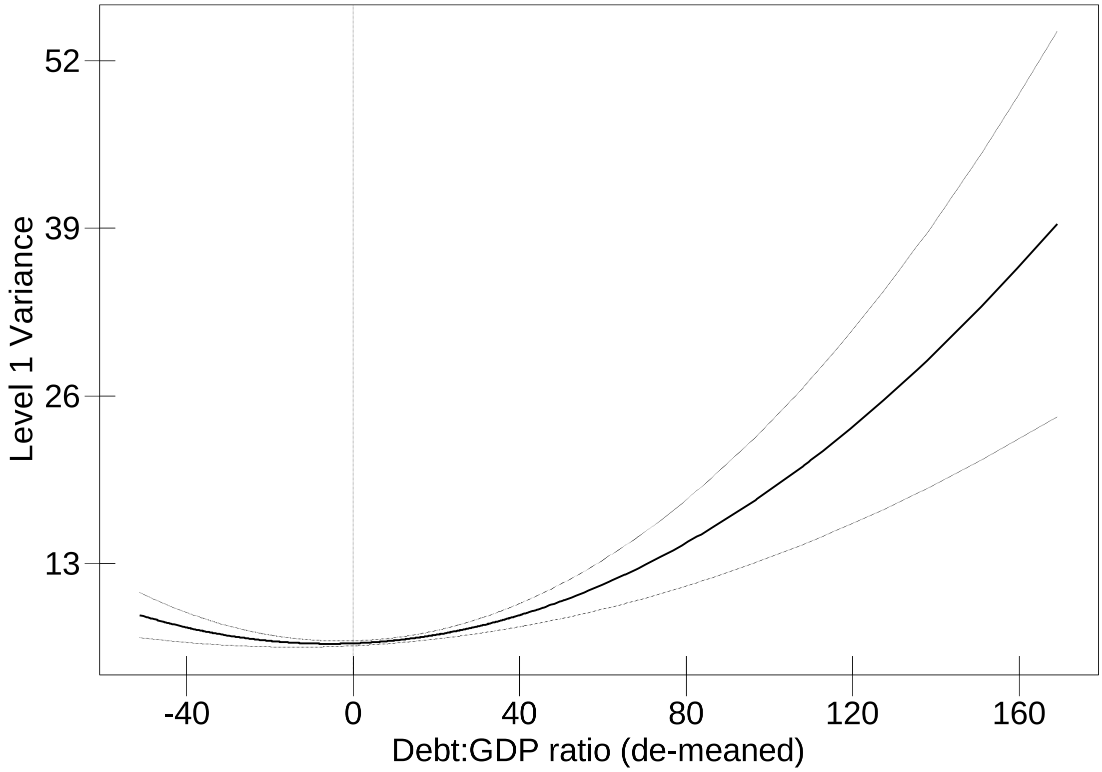


Figure 9 - Variance functions, from model 7, at level 1. With 95% confidence intervals.

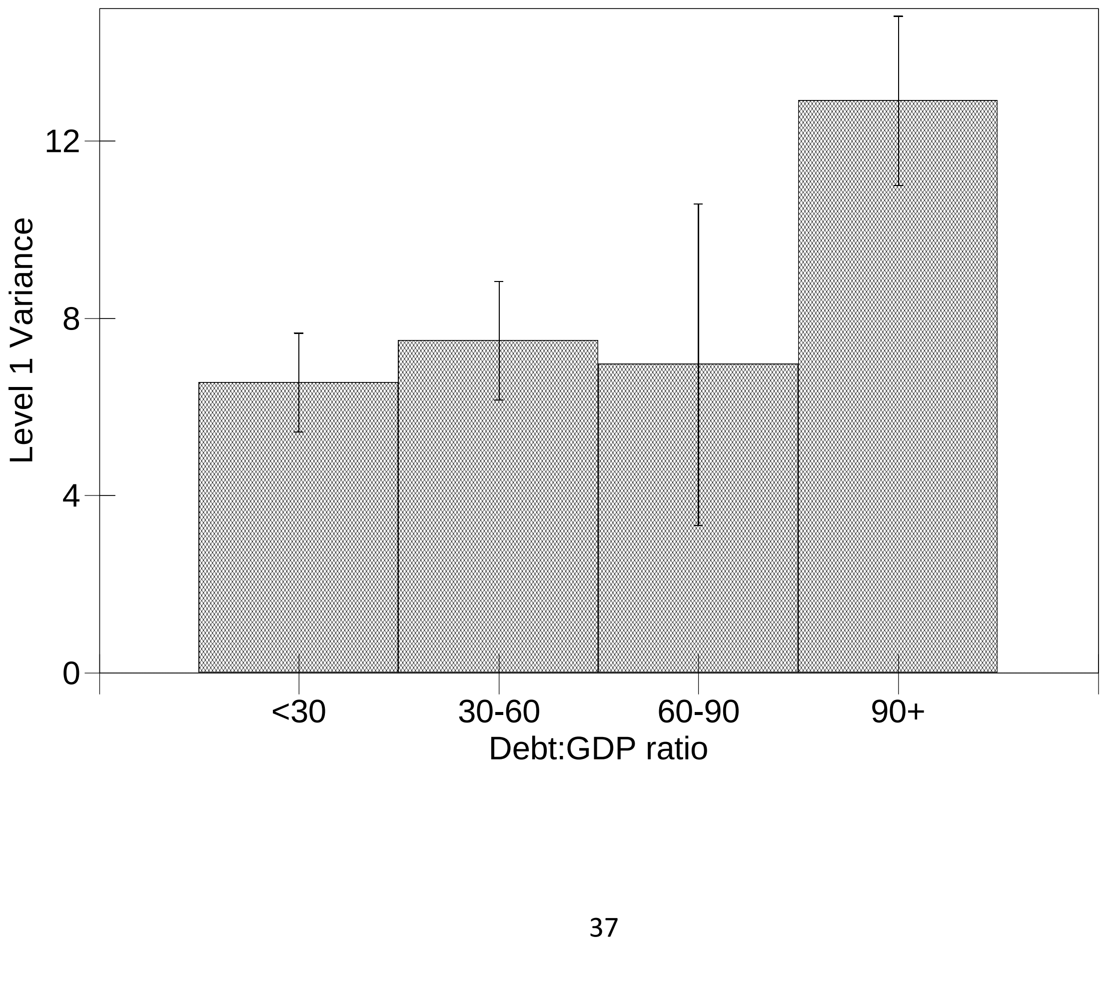


Figure 10: Impulse response graphs for each country from the multilevel distributed lag model. With 95% confidence intervals.

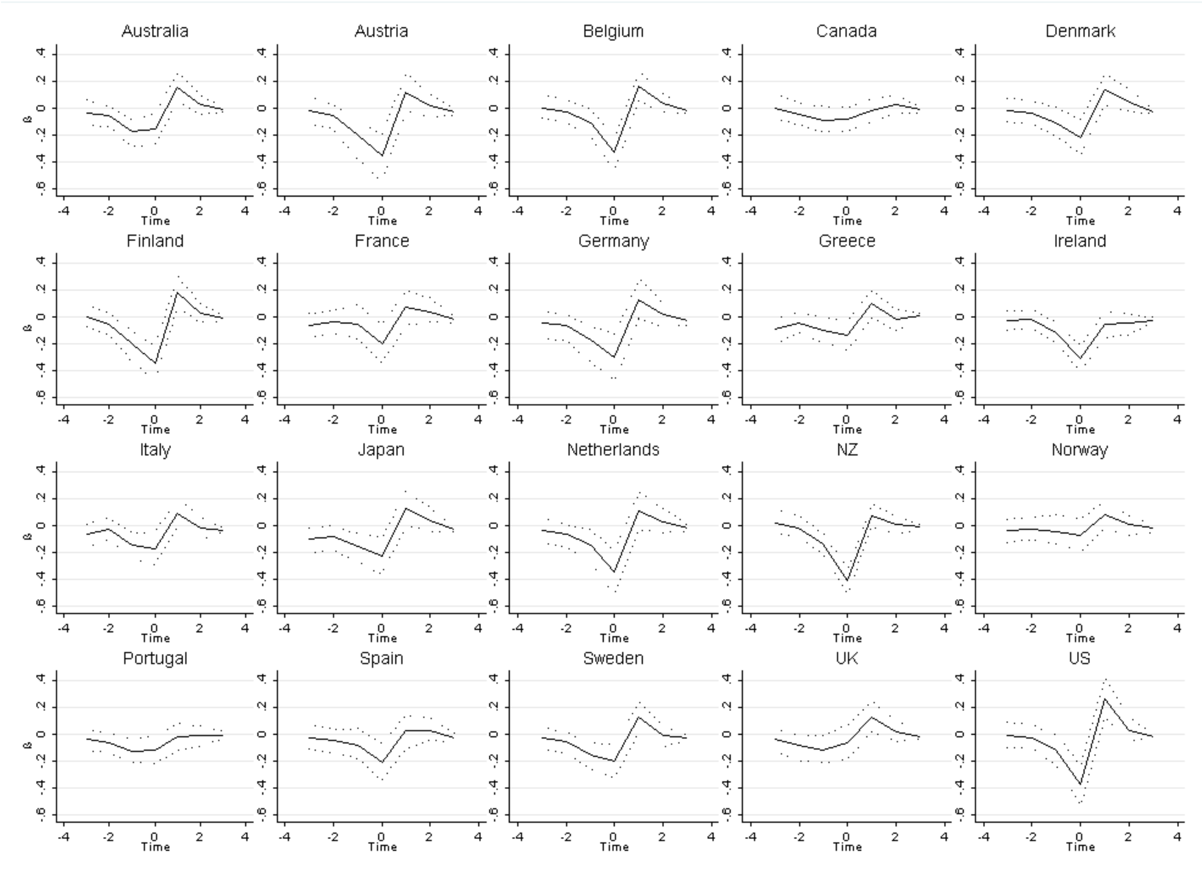


Table 1: Parameter estimates from models 1-4 (debt treated as continuous).

	Model 1		Model 2		Model 3		Model 4	
	B	S.E.	β	S.E.	β	S.E.	β	S.E.
Fixed Part								
Constant	3.413	0.110	3.410	0.116	3.413	0.117	3.448	0.149
Debt:GDP ratio (within)	-0.021	0.003	-0.021	0.009	-0.013	0.005	0.004	0.007
Debt:GDP ratio (between)	-0.012	0.006	-0.011	0.007	-0.012	0.007	-0.010	0.009
Year					-0.055	0.005	-0.062	0.013
Random Part								
Country Level								
σ_{u0}^2 (Cons)	0.097	0.094	0.141	0.084	0.146	0.077	0.310	0.145
σ_{u0u1}			-0.004	0.005	-0.004	0.002	-0.004	0.005
σ_{u1}^2 (within Debt)			0.001	0.001	0.000	0.000	0.001	0.000
σ_{u0u2}							0.000	0.010
σ_{u1u2}							-0.001	0.001
σ_{u2}^2 (Year)							0.003	0.001
Occasion Level								
σ_{e0}^2 (Cons)	8.533	0.354	7.531	0.381	6.78	0.348	4.194	0.318
σ_{e0e1}			0.009	0.008	0.008	0.008	-0.017	0.007
σ_{e1}^2 (within Debt)			0.001	0.000	0.001	0.000	0.001	0.000
σ_{e0e2}							-0.042	0.012
σ_{e1e2}							-0.002	0.001
σ_{e2}^2 (Year)							0.007	0.001
DIC:	5864.093		5799.334		5703.751		5513.136	

NB – all variables centred on their grand means

Table 2: Parameter estimates from models 5-8 (debt treated as categorical).

	Model 5		Model 6		Model7		Model 8	
	β	S.E.	β	S.E.	β	S.E.	β	S.E.
Fixed Part								
Cons	4.194	0.16	4.121	0.303	3.899	0.28	3.575	0.281
Debt (ref category is Cat 1, <30)								
Cat 2 (30-60)	-1.104	0.208	-1.053	0.412	-0.523	0.338	-0.076	0.371
Cat 3 (60-90)	-1.04	0.264	-1.046	0.466	-0.697	0.437	-0.146	0.442
Cat 4 (90+)	-2.085	0.333	-2.024	0.486	-2.117	0.709	-0.782	0.598
Year					-0.058	0.005	-0.058	0.012
Random Part								
Country Level								
σ^2_{u0} (Cons)	0.082	0.083	1.215	0.603	0.975	0.441	0.957	0.438
σ_{u0u1}			-1.427	0.748	-0.753	0.429	-0.807	0.473
σ^2_{u1} (Cat 2 Debt)			2.307	1.116	1.182	0.551	1.541	0.734
σ_{u0u2}			-1.645	0.866	-1.215	0.6	-1.165	0.602
σ_{u1u2}			2.421	1.191	1.219	0.645	1.46	0.771
σ^2_{u2} (Cat 3 Debt)			2.822	1.506	2.17	1.034	2.181	1.034
σ_{u0u3}			-1.081	0.627	-0.633	0.662	-0.717	0.506
σ_{u1u3}			1.528	0.812	0.494	0.707	0.75	0.649
σ_{u2u3}			1.795	1.053	0.825	1	1.15	0.807
σ^2_{u3} (Cat 4 Debt)			1.252	0.895	3.011	2.031	1.844	1.161
σ_{u0u4}							-0.006	0.015
σ_{u1u4}							-0.002	0.021
σ_{u2u4}							0.005	0.023
σ_{u3u4}							-0.011	0.026
σ^2_{u4} (Year)							0.002	0.001
Occasion Level								
σ^2_{e0} (Cons)	8.524	0.356	7.439	0.519	6.538	0.45	5.086	0.466
σ_{e0e1}			0.342	0.373	0.451	0.336	-0.064	0.326
σ_{e0e2}			0.009	0.459	0.222	0.415	-0.632	0.404
σ_{e0e3}			2.734	0.941	2.801	0.953	0.194	0.702
σ_{e1e4}							-0.035	0.037
σ_{e2e4}							-0.033	0.04
σ_{e3e4}							-0.088	0.039
σ_{e0e4}							-0.033	0.032
σ^2_{e4} (Year)							0.007	0.001
DIC:	5863.298		5815.161		5709.028		5536.908	

Table 3 – Parameter estimates from the multilevel distributed lag models.

	Year uncontrolled		Fixed Year controlled		Fixed and random Year controlled	
	β	SE	β	SE	β	SE
Fixed Part						
Cons	4.671	0.307	75.689	9.888	70.667	10.509
Debt t+3	-0.050	0.025	-0.030	0.025	-0.031	0.026
Debt t+2 (differenced)	-0.053	0.027	-0.046	0.026	-0.046	0.026
Debt t+1 (differenced)	-0.136	0.030	-0.128	0.029	-0.128	0.029
Debt (differenced)	-0.240	0.039	-0.230	0.038	-0.229	0.039
Debt t-1 (differenced)	0.089	0.031	0.099	0.033	0.101	0.033
Debt t-2 (differenced)	-0.004	0.022	0.015	0.023	0.016	0.023
Debt t-3 (differenced)	-0.028	0.006	-0.018	0.006	-0.017	0.006
Year			-0.036	0.005	-0.032	0.006
Random Part						
Country Level						
σ^2_{u0} (Cons)	1.313	0.626	0.694	0.352	6.933	22.560
σ^2_{u0} (Debt t+3)	0.004	0.003	0.004	0.003	0.004	0.004
σ^2_{u0} (Debt t+2)	0.004	0.004	0.003	0.003	0.003	0.003
σ^2_{u0} (Debt t+1)	0.007	0.006	0.007	0.006	0.007	0.006
σ^2_{u0} (Debt)	0.020	0.011	0.020	0.010	0.020	0.011
σ^2_{u0} (Debt t-1)	0.010	0.006	0.011	0.007	0.012	0.007
σ^2_{u0} (Debt t-2)	0.002	0.002	0.003	0.003	0.003	0.003
σ^2_{u0} (Debt t-3)	0.001	0.000	0.000	0.000	0.000	0.000
σ^2_{u0} (Year)					0.000	0.000
Occasion Level						
σ^2_{e0} (Cons)	4.146	0.193	4.009	0.189	3.984	0.187
DIC	4497.610		4460.760		4460.610	